selection has no tall varieties to work upon; or, if natural selection has eliminated them at that altitude, why has it not done so at a lower one? M. Bonnier's experiments go to show that they will not form there (see his figures and descriptions in "Rev. Gén. de Bot.," ii. p. 528). All I ask for is, that Mr. Cockerell's supposition should have some *facts* to rest upon.

(4) I did not question the fact that dwarf Alpine plants may have additional warmth from their close proximity to the soil. De Candolle observes: "Il en résulte que la couche superficielle du sol doit en général être plus chaude sur les montagnes que dans la plaine, la moyenne extérieure étant supposée semblable" ("Géog. Bot.," ii. p. 260). Indeed, I have grounds for suspecting that the prostrate condition, common in Alpine and other exposed plants, is generally, a result of the direct response of thermotropism. In support of this statement, I will give two out of several observations. *Malva sylvestris*, when growing freely, surrounded by a dry hard soil which is readily heated, develops a prostrate form; but it grows erect when in a damp soil and surrounded by other plants. Bluebells often lay their first leaves flat on the ground. The temperature *on* the soil and under such a leaf was $47^{\circ}\cdot5$ at 10 o'clock this morning (April 15). The plant is growing in shade and in damp soil. The temperature of the air 3 inches above it was $44^{\circ}\cdot5$. Lastly, the temperature *under* the flat leaves of a plantain, growing in short grass covered with dew, was $52^{\circ}\cdot5$, while at 3 inches above it, it was $49^{\circ}\cdot5$. What I questioned was the right of building an argument upon a statement, when the opposite condition of great cold arising from radiation at night is ignored. Thus, De Candolle observes:—"Plus on s'élève, moins les rayons solaires sont interceptés par l'atmosphère, plus aussi la déperdition de chaleur par le rayonnement nocturne et par l'évaporation est considérable" (*l.c.* ii. p. 259).

(5) Individual plants may be sheltered; but is not exposure the *prevailing* feature in high regions? Such is, at least, what I have observed.

(6) "Those plants which naturally grow quickly . . . . would survive." Quite true; all I maintain is that the primary cause is not *internal*, as "naturally" seems to imply, but *external* to the plant. That is, the total amount of heat and light received by the plant in a comparatively short time is what induces the rapid development of high Alpine and Arctic plants. But, on the other hand, the responsiveness of plants is not absolutely alike in all cases; therefore we have enough evidence to draw the deduction which I will express in Mr. Cockerell's words: "Given such variations [in the responsiveness to climate], is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short?" Certainly, as M. Verlot shows (*l.c.* p. 48), what horticulturists are familiar with, that precocious and late varieties are hereditary.

Mr. Cockerell's reply does not combat my sole contention, that it is unscientific to form theories of what "may be" and "might be," when *no, or insufficient, facts* are brought forward on which the supposition is based. There may not be a sufficiency to justify one in unhesitatingly accepting any particular theory; but all I ask for is something *positive*, or well-determined *facts*, upon which a hypothesis may be based. Science is not advanced, but hindered, by "may be's" and "might be's."

GEORGE HENSLOW.

Co-adaptation and Free Intercrossing.

As it appears to me, from his reply, that Prof. Meldola's views on the subject of "co-adaptation" are really the same as my own, I write once more in order to point out the identity.

In the first place, I of course agree that "every [considerable] variation, of whatever kind, must of necessity entail numerous structural and functional modifications"; and therefore "that there is not any 'fortuity' in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual." "But," as Prof. Meldola continues, "the case we have to consider is that in which the variations, A, B, C, D, &c., are not [thus] physically or physiologically correlated; but in which they are supposed to be 'independent variables.'" Now, this being the only case we have to consider, Prof. Meldola says, "The chances against such variations occurring in any one individual are, I concede most fully, 'infinity to one.'" And lower down he as fully concedes the consequence, that if it be supposed necessary for the utility of A that A should occur in association with B, C, D, &c., then the chances must be infinity to one against the perpetuated existence of A. But this is all that Broca's and Spencer's "difficulty" from co-adaptation alleges; and, as I said in my last letter, if it is to be met, it can only be so by denying that any cases do actually occur in nature where the utility of A is thus dependent upon the association of A with the other independent variables, B, C, D, &c. Accordingly, this is the way in which Prof. Meldola's reply does meet the "difficulty." For he concludes—and very properly—"if it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*"—*i.e.*, to show cases where the utility both of A and of B is dependent on the association A + B. And he goes on to add, "that the particular case of co-adaptation quoted by Mr. Pascoe [*i.e.* that of the giraffe] may resolve itself into a 'confluence of adaptations.'" But, if so, *cadet quæstio* as regards this particular case. And similarly, he continues, as regards all other possible cases; for the next sentence is,—"In fact, I do not think it would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race."

Thus Prof. Meldola meets the "difficulty" in the way which I believe is the only way that it can be met, viz. by denying that, as a matter of fact, any cases of "co-adaptation" (as distinguished from a "confluence of adaptations") ever do occur in nature. This, however, raises a distinct question. The issue between him and those who advance the "difficulty" will now no longer be logical, but biological. And, as stated in my previous letter, I do not intend to go into this distinct question. My only object has been to show that, unless the supposed fact of co-adaptation be denied, any appeal to the analogy of artificial selection is illogical; while, if this supposed fact is denied, any appeal to the analogy—then "real" enough—is superfluous. For, as I said before, it is self-evident that if each of the independent variables is of utility *per se*, natural selection will, sooner or later, blend them all together through free intercrossing.

Thus, with regard to the matter of co-adaptation, it appears that we are in complete agreement. Unless any given case—such as that of the giraffe—is denied to be a real case of co-adaptation, Prof. Meldola "fully concedes" that the chances against its evolution by natural selection alone must be "infinity to one," and, therefore, that the analogy of artificial selection as regards that case would be false. I am sorry to observe, however, that such agreement does not appear to prevail with regard to the distinct and much more important "difficulty of the swamping effects of intercrossing." For in his last paragraph Prof. Meldola ranges himself on the side of the "neo-Darwinians" as regards this difficulty, remarking, on the one hand, that he does not believe it to be "real," and, on the other hand, that it has been "amply treated of by Mr. Wallace and others." The difficulty here alluded to is to conceive how it is so much as logically possible for natural selection *alone* to preserve and accumulate incipient variations in the face of free intercrossing. "Mr. Wallace and others" have attempted to show that this is possible by pointing to the simple and well-known fact that "no being on this earthly ball is like another all in all." They tell us that every specific character—be it of structure, colour, form, size, strength, fleetness, and so forth—is variable round the specific average or mean; so that, if it be desirable either to augment or to diminish any given specific character, there will always be one-half of the number of individuals composing the species which are varying in the required direction at the same time. Now this, of course, is sufficiently obvious; but what does it prove? It only proves what nobody, so far as I am aware, has ever dreamed of doubting—viz. that if it were to become a matter of importance in the struggle for existence that the human form, for example, should be increased as to height, short men and women would be eliminated by natural selection, while tall men and women would transmit their tallness, until in the course of many generations the race would develop into a race of giants. All this is what I would call "pure Darwinism"; and it is not apparent to me how its re-statement by "Mr. Wallace and others" has in any way "broadened" our "ideas" upon the subject. But what I have called "neo-Darwinism" is the Darwinism which fails to distinguish between such a case of the *transmutation* of a specific type in

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a single line of change, and the *differentiation* of a specific type in two or more lines of change. The race of giants is formed—if I may again quote the pre-Darwinian expressions of the Laureate —by successive generations progressively mounting to higher things on the steps supplied by their own "dead selves." But this ladder-like succession of species in time differs in many respects from a tree-like multiplication of species in space; and the respect in which it differs most has reference to "the difficulty of the swamping effects of intercrossing." The transmutation of species in a single line of change is self-evidently capable of being effected by natural selection alone, because here free intercrossing among the uneliminated "fittest" is the very means whereby the transmutation is effected. But the tree-like multiplication of species (or Darwin's "divergence of character") is no less self-evidently incapable of being effected by natural selection alone; it is not so much as logically possible that any arborescence of species can take place unless natural selection is assisted by some form of isolation at the origin and throughout the development of every branch. The race of giants is evolved by intercrossing being *permitted* between all the uneliminated individuals of each successive generation; but if at the same time a race of pygmies, another race of blacks, another of whites, &c., are to be formed, it is a plain necessity of the case that intercrossing must be *prevented* between all these different races; else natural selection could not so much as begin to differentiate them.

This all-important distinction between what Mr. Gulick has concisely termed "monotypic" and "polytypic" evolution is not observed by "Wallace and others," who are now said to have "amply dealt" with the "difficulty" in question. For they deal only with the case of monotypic evolution, with reference to which no one has been so foolish as to allege any difficulty; and thus their whole treatment of the subject is irrelevant to the only difficulty which has been alleged, viz. the abstract impossibility of natural selection having ever effected *polytypic* evolution, or *divergence* of character, without the co-operation, in some form or another, of segregate breeding by the prevention of free intercrossing.

George J. Romanes.

Oxford, April 17.

The Flying to Pieces of a Whirling Ring.

Dr. Lodge appears to be still in error in making (p. 534), without qualification, the statement that "the dangerous tension will be set up in a straight portion of any endless band running in the direction of its length with the critical speed $\sqrt{\left(\frac{T}{\rho}\right)}$, by the agency of the curved portions which necessarily exist somewhere." For the curved portions of such a band might run *inside* smooth guides which would supply the necessary centripetal pressure and relieve the band from all tension. Such an arrangement was in my mind when I wrote (p. 463) that the band *need not* be stretched to its breaking strain. It is true that Dr. Lodge began his original letter (on p. 439) by specifying a ring "not radially sustained," but he will, I feel sure, allow me to call attention to the fact that without a repetition of this qualification his remark that I have quoted is incorrect: and I think he will see that his supposition that my remarks referred only to a straight bar with free ends, and not to an endless band, is contrary to the expressed terms of my letter. The fact remains that the motion of even an endless band has not necessarily anything to do with the stability of the straight part. Dr. Lodge apparently still wishes us to suppose otherwise.

Devonport, April 12. A. M. Worthington.

A Lecture Experiment illustrating the Magnetic Screening of Conducting Media.

A Lecher's tube (*Wied. Ann.*, xli. p. 850) is put, by means of two cork rings, into another large (4 cm. diam.) glass tube, with a crane on one end. Holding the tube in one hand, and approaching it to a wire in which electrical waves are produced, a continuous lighting in the Lecher's tube is to be seen. This lighting remains even if the large tube, including the Lecher's tube, is filled with water. But if the tube is filled with dilute sulphuric acid the light disappears.

When we open the crane and the acid flows out, we perceive the light again, but only in that part of Lecher's tube which is not surrounded with the acid. The light in the Lecher's tube penetrates but very little below the level of the acid in the surrounding tube.

J. J. Borgman.

St. Petersburg, University, April 14.

The Intelligence of the Thrush.

It is, I think, well to record the following observations of the intelligence of the thrush. The first happened on June 28, 1865. I then saw, from the windows that look out on the little lawn north of my house, a thrush steadily "stepping westward" in front of the hedge that parts the lawn from the public road. The bird seemed to be intentionally making for a gravel path that, after passing almost close to the windows, bends to the north-west, toward the small gate of my front garden. It was bearing something in its bill. On coming to the path it attempted to break this on a stone. It did not succeed. It then tried another stone. This time it succeeded. Thereupon it flew away. On the spot I found a remarkably big stone embedded in the path, and round it were scattered bits of snail shell. The bird had eaten the snail.

The second of the observations I would note, and the more striking of the two, happened on June 5, 1890. I then was viewing the gravel path from the westernmost of the four windows. Just beneath me, standing on the path, was a female thrush. She had succeeded in breaking a snail shell. She had the snail in her bill. But, despite of vigorous efforts, she could not swallow it. Up hopped a male thrush. Standing before the female, he opened his bill. She dropped the snail into his bill. He chewed the snail. He dropped it back into the female's ready bill. She swallowed it. The pair blithely trotted off, side by side, toward the small gate. I saw them no more.

John Hoskyns-Abrahall.

Combe Vicarage, near Woodstock, Oxfordshire,
April 16.

Cocks and Hens.

Whilst I was living at Highfield House in Nottinghamshire in 1879, a duck-wing game bantam injured her leg after she had been sitting for a fortnight, and could no longer remain on the eggs. The cock bird, however, took her place, and not only hatched the brood, but acted in all respects like a hen, brooding the young, setting his feathers up like a brood hen, and using the same peculiar cluck that a hen has for calling her chickens, also sleeping on the nest instead of his usual perch—being, in fact, a mother for the time. Years before (in 1841) one of my Dorking hens was accidentally killed, leaving a brood of chickens some five weeks old; the Dorking cock took to them and brought them up, but in this instance there was no change except the act of brooding.

E. J. Lowe.

Shirenewton Hall, Chepstow, April 12.

Cackling of Hens.

It is often difficult to recall an actual instance of what may be a matter of very common occurrence. Such is to a certain extent the case with the subject to which Prof. Romanes's query in Nature of April 2 (p. 516) refers.

In a general way it is my impression that the cackling of jungle fowl is not very commonly heard in India, but I feel certain that I have heard it occasionally, and that I once did hear it upon a somewhat considerable scale is impressed very distinctly upon my memory by certain and special circumstances. My tent for a few days in April 1876 was pitched close to a perfectly impenetrable patch of thorny jungle in Orissa. This cover was full of jungle fowl, and I remember hearing the cackling of the hens, which reminded me of the familiar farm-yard sounds of home. It is possible that in this case the safety of their retreat may have had something to do with their not fearing to cackle with unusual vigour.

V. Ball.

Science and Art Museum, April 18.

THE PARADOX OF THE SUN-SPOT CYCLE IN METEOROLOGY.

In 1872 and 1873, three writers, independently of each other and dealing with different branches of meteorological science, revived an old speculation of Sir William Herschel's, that the sun's heat probably varies with the

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